We thank the referee for the valuable and thoughtful comments which have helped to improve the paper. Our responses to the comments are below.

1. abstract, line 20, "the cloud thickness effect is positive for moderate/heavy drizzling clouds": only one single simulation showed a positive cloud thickness effect so how do you justify extrapolating to this conclusion? At least correct this statement to say "..for a moderate/heavy drizzling cloud".
>> Response: We have corrected the statement as follows: "positive for a moderate/heavy drizzling cloud".

2. abstract, line 23: I don't personally feel the concept of "cloud susceptibility" is well enough known to refer to it here without explaining what it actually is.
>> Response: We have revised the statement as follows: "The sign and magnitude of the Twomey effect, droplet dispersion effect, cloud thickness effect, and cloud optical depth susceptibility to aerosol perturbations (i.e., change in cloud optical depth to change in aerosol number concentration) are evaluated by LES experiments and compared with analytical formulations."

3. p15502: in this discussion of the possible relative impacts on cloud top and base heights, and thence on whether the cloud thins or thickens, the study of Randall (1984) is highly relevant.
>> Response: We thank the referee for pointing out this paper. We have added the statement in Sec. 5.1 as follows: "Cloud-top entrainment tends to raise the cloud base by diluting the cloud with warm and dry air, but it also tends to lift cloud-top height (e.g., Randall, 1984)."

4. p15503, line 15: "In order to obtain a comprehensive view of these [aerosol cloud-precipitation] interactions, high resolution LES simulations are carried out". Strictly you could just use a Lagrangian parcel model, for example, to study aerosol-cloud-precipitation interactions! LES allows the interactions with the turbulent dynamics to be studied.
>> Response: We have revised the statement as follows: "Taken as a whole, a number of studies essentially cover the range of aerosol-cloud-precipitation interactions. No single study, however, covers the spectrum of aerosol and meteorological influences relative to a consistent base case. High-resolution LES simulations that investigate a full range of aerosol and meteorological variables are carried out in the present study."

5. p15504, line 14: I assume you mean the "cloud droplet profile tends to be subadiabatic", otherwise I don't know what you mean?
>> Response: We have corrected it as "sub-adiabatic".

6. p15504, Eq (2) and subsequent text, and section 2.4: this factor of $(1+f)^{(2+m)/3}$ is introduced to represent the effects of a subadiabatic profile, which is fine, but given section 2.4 dismisses these dependencies ("this term cannot be evaluated separately ...the effect of diabaticity is intertwined with all the previous effects") there appears no need or justification for its complexity. The second part of the statement in line 19 also makes no sense to me. Cloud-free is not a natural opposite state to adiabatic! In what way, physically, are you thinking of approaching cloud free conditions? As stratocumulus breaks up the in-cloud profile could still be reasonably close to adiabatic and yet the cloud cover decreases. Mathematically you can think of the liquid water gradient reducing from adiabtic to zero, at which point you have approached cloud free, assuming zero cloud water at cloud-base, but that is not a physical limit. Why not simplify this whole section by introducing only a factor g, say, to (2) that equals 1 for an adiabatic layer and reduces as the degree to which the profile is sub-adiabatic increases?
>> Response: We have revised the statement as follows: "Equation (1) can be generalized as

$$\tau = \frac{9}{10} (\frac{4}{3}\pi)^{\frac{1}{3}} l_0^{\frac{2}{3}} f^{\frac{2+m}{3}} (k\, N_{ad})^{\frac{1}{3}}\ H^{\frac{5}{3}} \qquad (2)$$

where f is 1 under adiabatic conditions, and approaches 0 as the degree to which the profile is sub-adiabatic increases.”

7. p15505, Eq (4): this equation is clearly making significant assumptions about the dynamics in the cloud layer as it only requires knowledge of the updraught velocity at cloud base. Given the point of this paper is to include dynamical interactions, through use of the LES, these assumptions should be discussed.
>> Response: In this analytical expression, the adiabatic cloud droplet number concentration is a function of aerosol number concentration, updraft velocity, c, and k_s. The updraft velocity is the single dynamical variable considered in this expression. And in Eq. (5), it is shown that the relationship between N_{ad} and N_a depends only on k_s. This analytical expression is simply used to illustrate the fitting parameterization by Twomey (1959), and is not applied in this LES study. In the simulation, the activated particles are calculated based on the activation scheme and microphysical processes described in Sec. 3.2. The LES results are compared with the analytical expression. We have added the statement as follows: “The updraft velocity is the single dynamical factor considered in the analytical expression.”

8. p15506, line 14, “the dispersion forcing”:the dispersion in the droplet distribution is responding to the N_a forcing so why do you refer to the dispersion as being the forcing agent?
>> Response: We have removed the word ‘forcing’.

9. p15506, line 24, “this trend is evident in in-situ measurements”: I don’t see how you can distinguish the dispersion effect from the Twomey effect in observations where both must occur simultaneously, without running an off-line radiation code where each aspect is altered independently. In which case the effect is evident in the radiative transfer calculations, not the observations.
>> Response: We have revised the statement as follows: ”This trend is evident from the calculation based on in-situ measurements by Miles et al. (2000) and individual ship tracks in Lu et al. (2007).”

10. p15508, line 11: for the present study it would be good if the WRF LES showed good agreement with observations for relevant dynamical fields but I note that Wang et al (2009) showed the WRF model had w_0^2 smaller than the entire range of LES in Ackerman et al (2009) where those LES already underestimated this compared to the observations. Similarly, comparing the WRF turbulence profiles in Fig.3 with the LES and observations in Fig.9 of Duynkerke et al, WRF is clearly very poor in the daytime with both w'^2 and the buoyancy flux greatly underestimated. Even at night, what has happened to the buoyancy fluxes in the subcloud layer in WRF? The observations show roughly constant values of 1.5 to 2.0 $\times 10^{-4}$ m^2 s^{-3} between cloud-base and the surface while in WRF it is 1 or less at cloud base and tends almost linearly to zero at the surface? A lack of turbulence in stratocumulus clouds would seem to me to be a serious weakness of an LES for studying cloud-turbulence interactions. This weakness should at least be discussed here.
>> Response: Since the sounding profile and large scale divergence rate are not the same as those in Duynkerke et al. (2004), and precipitation is excluded in their study, the direct comparison between our results and Duynkerke et al. (2004) may not be appropriate. In our simulation, as the water vapor mixing ratio is lower and the subsidence is stronger compared to those of Duynkerke et al. (2004), the LWP is lower during the nighttime. Lower LWP (thinner cloud) results in less radiative cooling and thus lower updraft velocity variance. Also, the updraft velocity variance decreases with existence of precipitation. During the daytime, the overestimation of SW radiation from the RRTM SW scheme causes the boundary layer to be overly heated and the cloud to be thinner, and thus a significant reduction of w'^2. The near zero buoyancy flux near the surface is a result of near zero surface sensible heat flux in the simulation. Nevertheless, it does seem that the WRF LES underestimates the mean updraft velocity variance (w'^2), as also revealed in Wang et al. (2009). We have clarified as follows: “Note that Wang et al. (2009) compared WRF LES with other models for the same intercomparison case study (Ackerman et al., 2009). Most model variables and derived quantities (e.g., total water mixing ratio, liquid water potential

temperature, LWP, buoyancy flux, total water flux, TKE, and cloud fraction) lie within the corresponding ensemble range in Ackerman et al. (2009); however, the variance of vertical velocity and below-cloud rain rate were underestimated by WRF LES for the case they considered."

11. p15508, line 20: what are the implications of this cutoff radius between cloud and rain drops?
>> Response: Each droplet has its own sedimentation rate; by setting the cutoff radius between cloud and rain drops, we can calculate the cloud water mixing ratio and rain water mixing ratio, as well as cloud droplet number concentration and rain drop number concentration. This helps to distinguish between cloud and rain drops. When the droplet radius is larger than 40 µm, it moves into the rain drop category.

12. p15509, line 13: it doesn't seem particularly realistic to hold the aerosol number concentration constant as it must surely evolve in reality with wash-out and cloud processing etc. What might be the implications of this assumption?
>> Response: In reality, the aerosols would experience cloud processing, such as washout, convective redistribution, coalescence processing (change in the aerosol number and size due to repeated drop coalescence events), etc. However, these are not included in the current model. Several models apply an aerosol regeneration scheme to account for completely evaporated cloud drops in sub-saturated air (e.g., Hill et al., 2008, 2009; Xue et al., 2010). Ignoring regenerated aerosol can result in an unrealistic decline in aerosol number concentration, thus underpredicting the droplet number concentration. Assumptions have to be made concerning the regenerated aerosol size distribution, as discussed in Xue et al. (2010). The regenerated aerosol particles are often assumed to have the same size distribution as that of the initial aerosol, which leads to bias. In this study, the number of CCN, activated at each time step, is equal to the difference between the particle number that would be activated at the diagnosed supersaturation and the pre-existing droplet number, consistent with several previous studies (e.g., Stevens, et al., 1998; Lu and Seinfeld, 2005, 2006; Sandu et al., 2008), allowing a consistent comparison with their results.

13. p15510, line 20: was any drizzle observed for this case? Drizzle was not represented in the Duynkerke et al study so it seems odd to have to reduce the total water mixing ratio in order to generate moderate drizzle? If the LES doesn't reproduce the observed precipitation with the observed mixing ratio it rather questions the validity of the LES for this microphysical study.
>> Response: In Duynkerke et al. (2004), the removal of liquid water by precipitation was not taken into account. Their simulations exclude precipitation, with the precipitation scheme turned off. Therefore drizzle was not represented in their study.

14. p15510, line 27: why was the divergence rate reduced from the Duynkerke et al study in the control?
>> Response: For the divergence rate, we use the same value as that of Hill et al. (2009).

15. p15512, line 23: what does "a cloud top predominantly defined by LW radiative cooling" mean?
>> Response: The sentence in question has been removed to avoid confusion.

16. p15512, line 25, "In the clean cloud, sedimentation causes the cloud base to lower as precipitation nears the surface": cloud base in stratocumulus is typically where the relative humidity reaches saturation and so the cloud base falls as the RH of the air below increases towards saturation. In the clean cloud case, cloudtop falls compared to the other cases indicating, presumably, reduced cloud-top entrainment. This reduced entrainment of warm dry air would usually increase the RH of the PBL and so lead to cloud base falling, as is observed. This has nothing to do with droplet sedimentation, though, which I suspect is a minor perturbation on what is really the result of changes in the PBL heat and moisture budgets. Similarly, as the next sentence goes on to claim, the cloud does not dissipate because the larger droplets fall out! What about the smaller droplets? Again, I suspect the cloud dissipates because of the thermodynamics of the environment to which it is intimately coupled.

>> Response: The cloud base (lifting condensation level) is defined as the height at which the relative humidity (RH) of an air parcel reaches 100% when it is cooled by dry adiabatic lifting. In the clean case, when the cloud is drizzling, drizzle evaporation under the cloud base may increase the RH below the cloud, maintaining a lower condensation level, hence a lower cloud base (Lu and Seinfeld, 2005). Also, as suggested by the referee, in the clean cloud with existence of drizzle, the decrease in entrainment results in a relatively moister and cooler MBL. Therefore higher MBL RH leads to lower cloud base. The cloud top also falls due to reduced entrainment. Here, we have revised as follows: "In the clean cloud, the drizzle evaporation below the cloud can moisten and cool the sub-cloud layer, increasing the relative humidity of the sub-cloud air, lowering the cloud lifting condensation level, hence lowering cloud base (Lu and Seinfeld, 2005). Also, the cloud-top entrainment decreases in the presence of drizzle, therefore the cloud top falls. The decreased entrainment drying/warming as well increases the MBL relative humidity and leads to a lower lifting condensation level." In the latter part of the comment, "larger droplets" actually refers to the size as compared to the polluted clouds (N_a 200 and 1000 cm^{-3}). The sentence has been removed to avoid confusion.

17. p15513, line 18: as with the preceding point, microphysical arguments are being used to explain what appear to be simple thermodynamic budget responses typical of the stratocumulus diurnal cycle. The cloud top falls due to reduced entrainment (in turn induced by SW heating stabilizing the cloud layer and so reducing TKE), cloud base rises simply because of the dominance of SW heating in the thermal budget reducing RH more than the reduced entrainment leads to increased RH.
>> Response: During the daytime, SW heating offsets the LW effect, leading to decrease in TKE and entrainment. Therefore the cloud top height becomes lower. And as the MBL gradually warms during the daytime, RH decreases and results in higher cloud base. We have revised the statement as follows: "With a stabilized MBL and decreased TKE during the daytime, the cloud top falls by 80 m due to reduced cloud top entrainment. As the MBL gradually warms with SW heating, the relative humidity in the MBL decreases, causing the cloud base to rise by 100 m."

18. p15515, line 5: if the rise in cloud top were to be due to increased SSTs warming the PBL and so reducing the inversion strength, then that should result in a gradual acceleration of cloud-top rise - initially the inversion is the same strength as the control and so should initially show the same entrainment rate. However, Fig.5b shows cloud top diverging from the outset. This suggests to me it is rather the increased surface fluxes themselves driving stronger TKE and thence stronger entrainment.
>> Response: Both surface latent and sensible heat fluxes increase with higher SST. This results in a warmer and moister MBL, and overall leads to lower relative humidity and thus higher cloud base. The enhanced surface fluxes also increase the turbulence and cloud top entrainment, thus deepening the cloud. We have revised the statement as follows: "As SST increases, the surface sensible and latent heat fluxes increase accordingly. The extent of heating exceeds the extent of moistening in terms of affecting the relative humidity, resulting in lower relative humidity under higher SST, and thus higher cloud base. The increased surface fluxes also enhance the TKE and cloud top entrainment, and therefore deepen the cloud by rising cloud top."

19. p15515, line 20, "smaller cloud droplets evaporate more efficiently": more confusion of thermodynamcis with microphysics I suspect, see my points above. What are the differences in droplet sizes being referred to and what does that imply in terms of difference in timescale for evaporation? Typically the difference in evaporation timescales would be tiny compared to the hours over which the cloud is dissipating, so how can they possibly be relevant as you suggest?
>> Response: The statement has been revised as follows: "In SST290 and SST292 polluted clouds, stronger evaporation-entrainment and sedimentation-entrainment drying/warming as compared to that in clean clouds further dries the MBL, leading to cloud dissipation at ~ 14 h with existence of strong solar radiation."

20. p15516, line 17: QFT1 is not the only clean case with cloud at the end of the simulation - what about SST292 in Fig.5?
>> Response: The following statement has been removed: “Among all the clean cases, QFT1 is the only one in which the cloud exists at the end of the simulation.”

21. p15517, line 11, “entrainment is weaker in this case”: but cloud base and surface moisture fluxes are unchanged. This indicates that the PBL T and moisture profiles must be very similar and so the heat and moisture budgets must also be very similar. Hence how can the entrainment rate have changed as this is a significant term in those budgets?
>> Response: The vertical velocity variance (a measure of strength of turbulent mixing) increases in the DIV3 polluted case due to increase in LWP and thus cloud top radiative cooling. The statement in question has been removed.

22. p15517, line 12, “during the second night the cloud grows even thicker with LWP > 200 g m^{-2}”: not in Fig.7e it doesn’t! The maximum is at most 160 g m^{-2}.
>> Response: We have corrected the statement as follows: “During the second night, the cloud grows even thicker, with LWP reaching 160 g m^{-2}.”

23. p15517, line 15, “the cloud becomes thinner due to stronger “capping” from the air above”: what do you mean by “capping”? If you mean because the stronger subsidence has resulted in a lower inversion height and thence cloud-top, then just say that?
>> Response: We have revised the statement as follows: “In the DIV8 case, on the other hand, the stronger subsidence results in lower inversion height and therefore lower cloud top height.”

24. p15517, final paragraph: this summary is simply confused! When D is decreased the cloud thickens and LWP increases in the short term (ie first 5 hours), as shown in Fig 7a, not the other way round.
>> Response: We have corrected the statement as follows: “when D is increased compared to the Control case, the cloud thins and LWP decreases on a short time scale; …”

25. section 5.2.4: were there any changes in cloud droplet number concentration as wind speed might have some impact on aerosol activation?
>> Response: The horizontal wind speed is changed for the whole domain, mainly affecting the surface fluxes as stronger wind helps ventilate the surface. The vertical wind speed does not change significantly, and thus the impact on aerosol activation is relatively small. Also, the change in cloud droplet number concentration is negligible.

26. section 5.3: why is only the difference between N_a of 100 and 1000 shown? The effect of changing aerosol has already been shown to be non-linear (eg. For the control, Fig 4a shows the LWP generally increases between 100 to 200 and decreases from 200 to 1000) and so this figure is rather misleading.
>> Response: As the nonlinear relationship has been discussed in Sec. 5.1, for the sensitivity studies with different environmental variables, we carry out only the simulations with N_a 100 and 1000 cm^{-3} to compare the differences between clean and polluted conditions. Only for four cases (Control, SST290, QFT3, and DIV3) N_a 100, 200, and 1000 cm^{-3} are all simulated; therefore LWP differences from N_a 100 to 200 cm^{-3} and 200 to 1000 cm^{-3} are not plotted separately. By showing the LWP difference between 100 and 1000 cm^{-3}, results from both precipitation suppression and enhanced entrainment can be demonstrated, as compared to Fig. 7 of Sandu et al. (2008).

27. p15519, line 3, “overall LWP is found to be more sensitive to precipitation than entrainment”: what is the justification for this? Doesn’t the greater number of cases with Δ LWP < 0 imply that entrainment related effects (that would be expected to reduce LWP, ie c,d,e in the introduction) are dominating over the precipitation effects (that would be expected to increase it)?

>> Response: The absolute value of ΔLWP due to suppressed precipitation (70 g m^{-2}) is larger than that due to entrainment drying/warming (~28 g m^{-2}). Therefore it is stated that LWP is more sensitive to precipitation than entrainment within the case considered. The sentence in question has been removed to avoid confusion.

28. p15523, line 11, "during daytime the ranges of values are more scattered due to the MBL decoupling": given that the key issue is what happens to the cloud SW albedo under aerosol changes, this suggests that the role of decoupling is a leading order mechanism that needs to be investigated more thoroughly!
>> Response: The following material, including the new figure, has been added to the paper.
"Certain processes, including the solar absorption, cloud top entrainment, reducing surface buoyancy fluxes, and drizzle evaporation below cloud base tend to promote a more stable density stratification within the MBL (Nicholls, 1984; Lewellen and Lewellen, 2002). Daytime absorption of solar radiation often leads to afternoon cloud thinning due to diurnal decoupling. Decoupling can occur when subcloud buoyancy fluxes become negative, inhibiting convection below cloud base (e.g., Bretherton and Wyant, 1997). The existence of decoupling can be diagnosed using the buoyancy integral ratio (BIR) (Turton and Nicholls, 1987; Bretherton and Wyant, 1997) defined as:

$$BIR = -\int_{\substack{z < z_{cb} \\ at\ which \\ <w'\theta_v'> <0}} < w'\theta_v{}' > dz \,/ \int_{all\ other\ z} < w'\theta_v{}' > dz$$

where θ_v is virtual potential temperature, z_{cb} is cloud base height. In Turton and Nicholls (1987), the value BIR > 0.4 is chosen as a condition for decoupling of the sub-cloud layer and the cloud layer. Bretherton and Wyant (1997) suggest that the threshold value BIR > 0.15 is more appropriate. BIR values under nighttime (4-7 h) / daytime (12-15 h) conditions and clean (N_a 100 cm^{-3}) / polluted (N_a 1000 cm^{-3}) clouds are shown below for eight cases.

If BIR > 0.15 is used for the decoupling threshold, the MBL under most daytime cases is decoupled. As the daytime solar heating offsets the cloud top radiative cooling, less production of turbulence by cloud-top cooling favors greater decoupling (e.g., Bretherton and Wyant, 1997; Stevens, 2000) and hence a thinning of the stratocumulus layer. The largest BIR is shown in DIV8 polluted case during the daytime condition. In the DIV8 polluted case, stronger subsidence and enhanced entrainment lead to a thinner cloud. Solar heating during the daytime further results in enhanced decoupling of sub-cloud layer and the cloud layer, which leads to cloud dissipation at ~ 14 h. In the WIND cases, stronger wind helps ventilate the surface. The surface latent heat flux, which is proportional to the mean wind, becomes more negative in the polluted cloud. This results in lower buoyancy flux near the surface, and enhanced decoupling of the sub-cloud layer and cloud layer. Thus BIR is higher under WIND polluted cloud during the daytime.

Under nighttime conditions, the MBL is well coupled, with BIR < 0.15 in all cases. However, the MBL under DIV3 and WIND clean conditions has slightly higher BIR than others, indicating that the heavier precipitation under DIV3 and WIND clean clouds leads to a more stable boundary layer and a less mixed/coupled MBL compared to those with lighter or no precipitation. This shows that below-cloud evaporation of drizzle produces a cooler and moister sub-cloud layer that inhibits deep mixing. Overall, it is shown that decoupling is most likely to occur during daytime conditions."

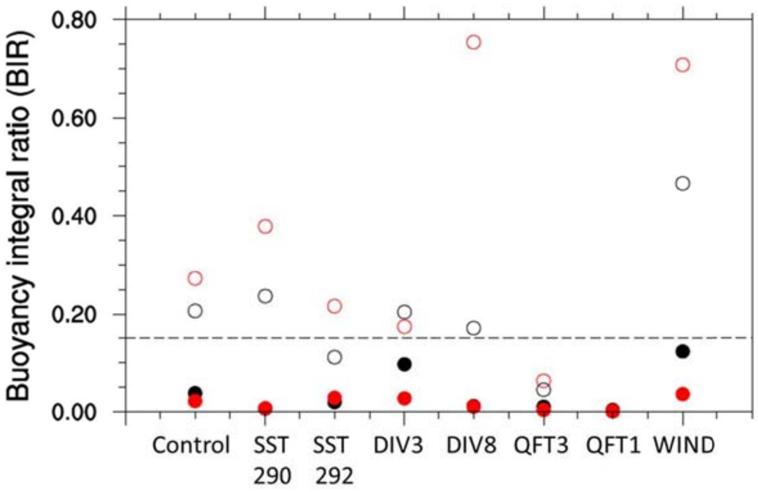

Figure. The buoyancy integral ratio (BIR) for clean (N_a 100 cm^{-3}) nighttime (4 – 7 h, black), clean daytime (12 – 15 h, black open circle), polluted (N_a 1000 cm^{-3}) nighttime (red), and polluted daytime (red open circle) clouds under different environmental conditions (Control, SST290, SST292, DIV3, DIV8, QFT3, QFT1, and WIND). The dashed line corresponds to critical value 0.15 (suggested by Bretherton and Wyant (1997)).

29. p15523, line 25: as with previous comments, why only consider changes in aerosol from 100 to 200 cm^{-3} when changes beyond that can have the opposite effect?
>> Response: We have added a new figure and paragraph to the paper, shown below, including changes from 100 to 200 cm^{-3} and 200 to 1000 cm^{-3} under both nighttime (4-7 h) and daytime (12-15 h) conditions for four cases (Control, SST290, QFT3, and DIV3). The same trend of R_{IE} versus cloud base height is shown as that in Wood (2007), with larger R_{IE} corresponding to lower cloud base and smaller R_{IE} corresponding to higher cloud base. The range of R_{IE} values during the nighttime (-0.42 to 0.20) is smaller than that during the daytime (-4.18 to 1.38), showing that R_{IE} is more scattered during the daytime. From N_a 100 to 200 cm^{-3}, R_{IE} is positive for both day and night conditions for the heavier drizzling cases (Control and DIV3), and is negative for the non/light drizzling cases (SST290 and QFT3). This suggests that with suppressed precipitation, R_{IE} tends to be positive (cloud thickens). From N_a 200 to 1000 cm^{-3}, R_{IE} is negative for all the cases considered, as the pronounced evaporation-entrainment and sedimentation-entrainment effects lead to cloud thinning.

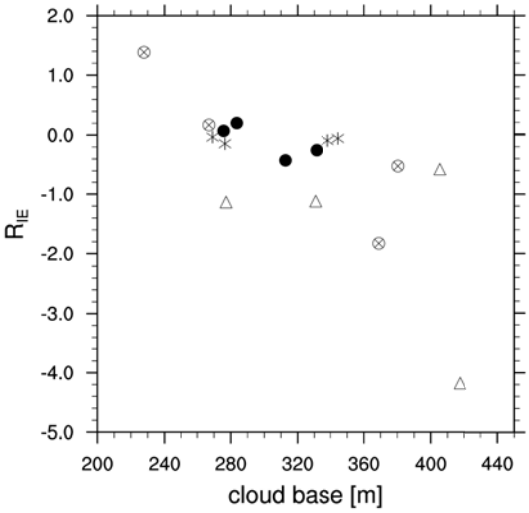

Figure. The mean ratio of second to first indirect effect (R_{IE}) for N_a from 100 to 200 cm^{-3} during nighttime (4 – 7 h, black filled circle) and daytime (12 – 15 h, circle with cross inside), and from 200 to 1000 cm^{-3} during nighttime (asterisk) and daytime (triangle).

30. p15523, line 26: again, the indirect effects are concerned with the cloud albedo and so why look only at nighttime data (hours 4-7)?
>> Response: Please see the response to the previous point (point 29).

The following new references have been added in the revised manuscript:

Lewellen, D. C. and Lewellen, W. S.: Entrainment and decoupling relations for cloudy boundary layers, J. Atmos. Sci., 59, 2966–2986, doi:10.1175/1520-0469(2002), 2002.

Nicholls, S: The dynamics of stratocumulus - aircraft observations and comparisons with a mixed layer model, Q. J. Roy. Meteor. Soc., 110, 783–820, doi:10.1256/smsqj.46601, 1984.

Randall, D. A.: Stratocumulus cloud deepening through entrainment, Tellus, 36, 446–457, 1984.

Stevens, B.: Cloud transitions and decoupling in shear-free stratocumulus-topped boundary layers, Geophys. Res. Lett., 27, 2557–2560,doi:10.1029/1999GL011257, 2000.